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I shall be most grateful for notice of any drawing not mentioned in the above list, whether published or unpublished, or for a reference to unpublished observations of this object, particularly of the central nebulous portions.

EDWARD S. HOLDEN.

United States Naval Observatory, Washington,
1877, *January* 31.

The Approaching Opposition of Mars—Request to Astronomers.

M. F. Terby, Doctor of Science, Louvain, Belgium, invites observers, particularly those in the equatoreal and southern regions, to make a large number of drawings of the planet *Mars* during the remarkable opposition of the present year, 1877. He recalls to them that he has published a Memoir, entitled "Aréographie, ou étude comparative des Observations faites sur l'aspect de la planète *Mars* depuis Fontana (1636) jusqu'à nos jours," t. xxxix. *Mem. Cour.* (4°) de l'Acad. R. de Belgique. This Memoir contains a large number of questions, which might be answered by the observers in the opposition of 1877; these refer to the principal doubtful points in Mr. Proctor's Chart. M. Terby begs the astronomers who have hitherto kindly communicated to him their observations to send him also their drawings of 1877; he hopes thus to perfect Mr. Proctor's already so excellent chart.

The Dunecht Observatory Publications.

Mr. Ralph Copeland writes to correct an error occurring in *Monthly Notices*, p. 199, where it is stated that the arrangement and reduction of vol. i. of the Dunecht Observatory Publications were performed by him. Although he is responsible for the general arrangement of the work, yet the whole of the computations were performed by Mr. Robert Copeland, of Manchester.

On Apparent Brightness as an Indication of Distance in Stellar Masses. By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope.

In the important investigations of the motion of the solar system in space by O. Struve and Airy, the distribution of the stellar distances has been made in accordance with the views of W. Struve. The correctness of these views has, from time to time,* been questioned. It may have been shown that some

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astronomers have attached undue importance to the numerical accuracy of the results obtained by W. Struve; but I cannot consider that the average distribution of the distances of stars according to apparent brightness has been, or indeed ever will be, disproved. I do not know that there is much novelty in my views, but the subject presents itself to my mind in the following way. It is certain that the apparent brightness of a star will, *cœteris paribus*, vary inversely as the square of its distance from us; and an inspection of any table of annual parallaxes will prove that we have around us ample space for very considerable variations in the magnitudes of stars from the sole effects of distance.

The following table, extracted from Herschel's *Outlines of Astronomy*, will be sufficient for my present purpose:—

Star's name.	Annual parallax.
	"
α Centauri	0·976
61 Cygni	0·348
α Lyræ	0·155
Sirius	0·150
1830 Groombridge	(0·71) doubtful
ι Ursæ Majoris	0·133
Arcturus	0·127
Polaris	0·067
Capella	0·046

Without attaching undue importance to the numerical accuracy of the smaller values, we have here a proved range of stellar distances in at least the proportion of 1 to 10. It is certain, therefore, that our brightest stars might be changed to stars of about the tenth magnitude by a mere shift of our relative position in space. But there are not the slightest grounds for our supposing that the relative distance of the stars from us is restricted to any such proportions as 1 to 10. Such a proportion merely marks the limit beyond which we cannot discriminate with much certainty between the distances of the stars. Annual parallaxes of less than a tenth of a second of arc are sensibly equal, because the limit of our present power of accurate measurement has been nearly reached. If we assume, therefore, that the magnitudes of the stars depend chiefly upon their distances from us, our assumption rests upon a *vera causa*, and we have independent proof of the sufficiency of the assumed cause to produce the observed differences in stellar magnitudes.

But I think it may be shown that such an assumption is not a mere barren hypothesis. I know of no more striking fact in Astronomy than the rapid increase in the number of the stars as we proceed down the scale of magnitudes. Every increase of

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optical power appears only to bring this fact into greater prominence.

If we accept distance as the principal factor in determining stellar magnitudes, the rapid increase in the number of the fainter stars not only becomes intelligible, but is a necessary consequence of our assumption on the most general distribution of stellar masses which we can conceive.

Suppose that there are n different degrees of intrinsic brightness in the stars around us. Suppose, for the purposes of numerical illustration, that we have over any surface these classes of stars distributed in any definite proportions. Since the brightness of a star is classed under discontinuous magnitudes, we can logically suppose, for our present purpose, that the stellar distribution is discontinuous also over the surfaces of small portions of spheres whose radii are such that a star shifted from one surface to the next would be changed one magnitude. With such a distribution of stars, without any assumptions of uniform distribution in all directions or at all distances, and with any proportions amongst the stars of different intrinsic brightness which could be admitted as probable, the large increase in the number of the fainter stars becomes a necessary consequence of the supposed distribution. I shall, however, for simplicity, introduce the conditions that the stars of different intrinsic brightness are to be supposed equally distributed, and their number in any given direction proportional to the area over which they are distributed. Such conditions are certainly not strictly true, and so far as they are untrue their tendency will be to make the results of the computations based upon them differ from the results of observation; but on the large average these conditions are probably close approximations to the truth. Through our position conceive a cone of small solid angle to be drawn in any direction. Let $x_1, x_2 \ldots x_n$ be the radii of the spheres which cut the cone at distances such that a star would be changed one magnitude by being shifted from one of the surfaces to the consecutive surfaces; x_1 being such a distance that the stars of the first degree of intrinsic brightness will at that distance appear stars of the first magnitude. Then, if $ds_1, ds_2, \ldots ds_n$ are the portions of the spheres intercepted by the cone, the number of the stars of the different magnitudes in the direction under consideration will be—

1st magnitude number $= \mathrm{C}$ (some constant).

2nd ,, ,, $= \mathrm{C}\left(1+\dfrac{ds_2}{ds_1}\right)$.

3rd ,, ,, $= \mathrm{C}\left(1+\dfrac{ds_2}{ds_1}+\dfrac{ds_3}{ds_1}\right)$.

nth ,, ,, $= \mathrm{C}\left(1+\dfrac{ds_2}{ds_1}+\dfrac{ds_3}{ds_1}+\dfrac{ds_4}{ds_1}+\ldots+\dfrac{ds_n}{ds_1}\right)$;

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for stars of the first degree of intrinsic brightness only appear of the first magnitude when found on ds_1; of the second magnitude on ds_2; and of the nth on ds_n.

Stars of the second degree of intrinsic brightness appear of the second magnitude on ds_1; of the third on ds_2; and of the $(n+1)$th on ds_n. Stars of the third degree of intrinsic brightness appear of the third magnitude on ds_1; of the fourth magnitude on ds_2, &c.

Consequently, therefore, since a similar result holds in all directions, we have generally the number of nth magnitude stars

$$= \mathrm{A}\left(1+\frac{ds_2}{ds_1}+\frac{ds_3}{ds_1}+\dots+\frac{ds_n}{ds_1}\right)$$

$$= \mathrm{A}\left(1+\overline{\frac{x_2}{x_1}}\Big|^2+\overline{\frac{x_3}{x_1}}\Big|^2+\dots+\overline{\frac{x_n}{x_1}}\Big|^2\right).$$

It remains to determine the numerical values of the ratios involved.

In Sir J. Herschel's volume of Cape Observations will be found some experiments showing that, if the double star α *Centauri* be taken as the standard, the brightness of stars of the first four magnitudes of the ordinary scale are very approximately

$$= \frac{1}{(1{\cdot}414)^2} \text{ for 1st magnitude stars.}$$

$$= \frac{1}{(2{\cdot}414)^2} \text{ for 2nd} \quad ,, \quad ,,$$

$$= \frac{1}{(3{\cdot}414)^2} \text{ for 3rd} \quad ,, \quad ,,$$

$$= \frac{1}{(4{\cdot}414)^2} \text{ for 4th} \quad ,, \quad ,,$$

Such a scale will not, however, be true for the fainter stars.

In the introduction to Carrington's *Redhill Catalogue of Stars*, there are some results of experiments with varying apertures, showing that from the fifth to the ninth magnitude of the ordinary scale the brightness of each magnitude is fairly represented by the relation

Brightness of nth magnitude $= 2{\cdot}75 \times$ that of $(n+1)$th.

I shall exhibit the proportional number of the stars of different magnitudes when the ratios of $\frac{x_n}{x_1}$ are determined with both these light ratios. We must expect, if the supposed distribution is any close approximation to the truth, to find that the computed numbers will agree with the observed numbers, for the first four magnitudes better with Herschel's scale, and for the last five magnitudes better with Carrington's scale.

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With Herschel's scale

$$\left.\frac{x_n}{x_1}\right|^2 = \left(\frac{n\cdot414}{1\cdot414}\right)^2;$$

And the number of nth magnitude stars

$$s_n = \frac{\mathrm{CA}}{(1\cdot414)^2}\{(1\cdot414)^2 + (2\cdot414)^2 + \cdots\cdots + n\cdot414)^2\};$$

$$s_n = \mathrm{C}\left(\frac{n^3}{3} + 0\cdot914\,n^2 + 0\cdot752\,n\right\}.$$

Next, assuming Carrington's light ratio, we have

$$s_n = \mathrm{C}\,\{1 + 2\cdot75 + (2\cdot75)^2 + \cdots\cdots + (2\cdot75)^{n-1}\};$$

$$s_n = \mathrm{C}\,\frac{(2\cdot75)^n - 1}{1\cdot75}.$$

The following Table exhibits certain numbers attributed to Argelander and the results of the two formulæ. The number of first magnitude stars is made equal for the purposes of comparison :—

Mag.	Argelander's numbers.	Computed numbers Herschel's scale.	Computed numbers Carrington's scale.	Mean of Compd.
1	20	20	20	20
2	65	78	75	76
3	190	195	226	210
4	425	390	642	516
5	1,100	683	1,786	1,234
6	3,200	1,094	4,931	3,012
7	13,000	...	13,581	...
8	40,000	...	37,360	...
9	142,000	...	102,780	...

It will be seen that the numbers based on Herschel's scale do agree best for the first four magnitudes, and those based on Carrington's scale best for the fainter magnitudes; whilst a mean between the two agrees best with the observed numbers for the end of Herschel's and the commencement of Carrington's scale. This is what might have been expected. The computed number of stars of the ninth magnitude falls short of the observed number by forty thousand; but when it is considered that this discrepancy is not greater than would arise from an uncertainty of estimation of about a tenth of a magnitude at the end of the scale, the agreement between the computed and observed numbers will, I think, be considered sufficiently close to prove that the supposed distribution of the stars must be a somewhat close approximation to the truth.

If the distribution which I have supposed be any close

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approximation to the truth, the average distances of the fainter stars must be greater than those of the brighter stars ; but it by no means follows that any one faint star is further from us than any one much brighter star. In fact, the supposition made has been that stars of all magnitudes may exist at all stellar distances.

On Observations of Contacts of the Limb of Venus, or Mercury, with that of the Sun. By Prof. Simon Newcomb.

During the past four years, I have had occasion to give some attention to the problem of obtaining accurate observations of the various phases of contact between the limb of an interior planet and that of the Sun. My views, as now presented, have been derived from the study of an artificial representation of a transit of *Venus*, aided by general considerations on the subject, and by the opinions and investigations of others. I am induced to present them at the present time by the researches of M. André, and the papers of Mr. Stone and Father Perry in the *Monthly Notices* for December last.

(1) If I might be allowed to criticise certain of the views of others in a general way, I should say that one defect of much of the reasoning on this subject is this : it has too generally been assumed that the geometric outlines of *Venus* and the Sun, considered as mathematical lines, can be noted in observation with the same sort of definiteness and precision as that with which the mind conceives them ; and sufficient attention has not been paid to the practical difficulties which the eye meets with in representing this geometric conception. I conceive that the question whether a certain phase can or cannot be definitely distinguished and observed by the eye is to be settled by actual trial, and by a consideration of the imperfections of vision, rather than by a consideration of its purely geometric definiteness of form.

(2) One result of the trials with the artificial transit is, that there is a certain phase near that of external contact which can be observed with the same order of precision as the internal contact, provided that the proper conditions are fulfilled. Among these conditions are, that the observer shall previously have practised on the artificial transit ; that he shall know at exactly what point of the Sun's limb to look for the first contact ; that he shall know when to look for the contact with an uncertainty of not much less than half a minute, nor much more than a minute ; and that he shall have a telescope of fixed size and power. Of course the phase thus observed will not be geometric contact, but that occurring at the time when the notch in the Sun's limb first becomes visible. This phase varies much less with variations in the atmospheric condition and in the size and power of the telescopes than might have been supposed.